On the supposed Unreliability of the Greenwich Observations of the Sun in Mean Results. By E. J. Stone, M.A., F.R.S.

In the *Monthly Notices* for November there appears a paper by my friend Mr. Dunkin on the "Greenwich Mean Tabular Errors of the R.A. of the Sun." The paper is an interesting one, as showing examples of considerable personalities between the different Greenwich observers. But the conclusion to which Mr. Dunkin is led that the apparent increase in the Tabular Errors in R.A. of the Sun since 1864 is due solely to these personalities, is one which appears to me unwarranted by the facts. The subject which Mr. Dunkin has attempted to treat is one of very great difficulty indeed.

With respect to personal errors which can be quantitatively determined directly, I am strongly of opinion that corrections should be applied to reduce each observer's work to the mean habit of all the regular observers; but with respect to personalities which can only be determined indirectly by assuming that the errors of the Tables disappear in the mean of a considerable number of results, I seriously doubt whether any great advantages are likely to result from their application.

But whatever may be the value of my opinion on the point, the usage at Greenwich has been to trust to mean results without any attempts, so far as I am aware, to correct for personalities except in the determination of clock-rate. The effect of this is to impress the mean habit of the general body of the observers upon the fundamental data; and consequently, although the Greenwich results are, in my opinion, thoroughly reliable and of great accuracy as mean results, the individual observations of a selected observer, instead of being free from personalities, as Mr. Dunkin seems to suppose, are generally more affected by such personalities than the general mean. This point has frequently been overlooked, but I believe that there is little doubt of its importance in the present case. The observation of the R.A. of the Sun consists of a difference between the times of transit of the centre, as determined from the times of transit of the limbs, and some one star corrected for clock-rate applied to some adopted Right Ascension of the star. In practice there will, of course, be usually several stars observed and the mean result taken. If, therefore, an observer has any distinct personality in such observations, this will prevent his so-called observed R.A. from agreeing with the tabular error given by the other observers, but the exact error which he gives will depend also upon the adopted Right Ascensions, and we can make it positive or negative, large or small, by adopting different values of the fundamental clock-stars.

Independently, therefore, of personal changes, there are to be considered the changes in the adopted clock-stars. Now, it has been the plan at Greenwich to discuss *all* the solar observations

of each year, for the determination of the corrections required by the adopted clock-stars, in order to refer them to the true mean equinox of the year. Such corrections to epoch are necessarily affected by the general mean habit of the body of the observers employed on the observations. From time to time the stellar observations of several years are collected and reduced to some common epoch, and corrected for the mean of the corrections deduced for the several years from the solar observations. These Sun observations have therefore become fundamentally incorporated in the Greenwich work; and if untrustworthy in mean results, the consequences are serious indeed. Now, the observations 1864 to 1869 were all based fundamentally upon the Right Ascensions of the 1860 Catalogue, which embraces the observations made from 1854 to 1860, and is affected by the mean correction to epoch of the observers who made these observations.

The observations 1870–1877 are based fundamentally upon the Right Ascensions of the 1864 Catalogue, and are affected by the mean correction to epoch of the observers during the period 1860–1867; whilst the observations made 1878–1881 are based on the standard places of the Nine-Year Catalogue, which embraces the observations made 1868–1876, and are affected by the mean habit of the observers during that period. It is clear, therefore, that if there have been such serious changes as those which Mr. Dunkin supposes to have taken place in the mean habit of the general body of the observers, then Mr. Criswick's isolated observations should have shown changes in the tabular errors of the Sun during the period 1864–1881, unless his personal habit has also changed. On the other hand, if there have been no such great changes in the mean habit of all the observers, then the mean errors in longitude are trustworthy results. The only real test of any considerable change in the mean habits of the observers is to see whether there is, or is not, any great change in the correction to epoch when stars are brought up from one epoch to another with precession constants and proper motions which are supposed to be known with sufficient accuracy not to seriously affect the observed differences. If we apply this test to the observations 1864–1869 and 1878–1881, we find the mean correction to epoch for the first period is $-0^{s}\cdot020$; whilst that for the second period is $-0^{s}\cdot015$. The difference is only $0^{s}\cdot005$ or $0''\cdot075$; and it is clear that there is, so far as we can thus check the point, no indication of any serious change of mean habit, such as Mr. Dunkin assumes to have taken place.

The principle of separating the different observers' work without referring to the mean equinox of each observer is, I feel certain, radically unsound. So far as materials exist for the inquiry, it appears clear to me that Mr. Criswick's observations do show an increase of tabular error between 1866·5 and 1879·5 when reduced to his own epochs. It is probable, but not quite certain, that the observations 1878–1881 when referred to their own equinox should be increased by about $0''\cdot24$ relatively to

those about 1864, on account of the addition now made to the reading for the line of collimation at Greenwich for effects of observations through the cubes.

So many changes were made during the period 1870 to 1877, and these changes have become so mixed up in the fundamental determinations, that it appears almost hopeless to attempt to disentangle their effects; but probably in mean results the errors are exceedingly small. It appears clear to me that unless we can trust the general mean results the Greenwich observations can afford us no reliable information on the question raised, and it is certainly illogical to assume that these Sun observations are sufficiently accurate to determine the corrections to epoch, and then afterwards to maintain that the mean results are unreliable.

The mean errors of the tables in longitude for the separate years, and the mean errors for the groups of years, 1864–1869, 1870–1877, 1878–1881, which result from the observations made by all the observers, are as follows:—

Year.	Mean Error.	No. of Obs.	Mean Error of Group.
	"		
1864	+0·025	122	
1865	+0·248	122	
1866	−0·109	109	
1867	−0·203	93	±0·080
1868	+0·245	131	
1869	+0·272	98	
1870	+0·122	126	
1871	+0·306	113	
1872	−0·037	120	
1873	+0·408	124	
1874	+0·762	109	±0·456
1875	+0·728	110	
1876	+0·675	112	
1877	+0·683	87	
1878	+0·816	85	
1879	+0·748	77	
1880	+0·861	115	±0·79
1881	+0·759	124	

On the Change in the adopted Unit of Time. By Major-General J. F. Tennant, R.E., F.R.S.

Mr. Stone's papers on the cause of the large error in our modern Tables, and the criticisms I have seen on them, have led me to consider the subject; and I put my idea on paper because it may simplify the question to some. The result is the same as Mr. Stone's, and the reasoning, though independently thought out, is in much the same line as part of his second paper in the *Monthly Notices.*

For many years past the use of the Sun for the purpose of regulating clocks in an Observatory has ceased. Practically all observations are made in Sidereal Time, and the Mean Time where required has been deduced from it; and even where series of observations have been used in the investigation for Tables the Times have been reinvestigated, the clock-errors having been determined by the observations of stars. Therefore the Mean Times used have essentially depended on the length of the mean solar day in seconds of sidereal time. The Julian year taken at 365·25 mean solar days therefore depends on this time. The relation between the solar and sidereal days has been practically till recently that determined by Bessel, and it is from observations dated in what we may call Besselian Mean Time that the investigations of physical astronomy have deduced the data of modern Tables.

One of the discoveries from these investigations has been that the mean motion of the Sun and the length of the mean solar day in sidereal seconds dependent on it were not sufficiently accurate. The result of introducing the new Tables has been that, while the sidereal day and second are unchanged, the unit of Observatory Mean Time has become the Leverrian solar day* and year instead of the Besselian, and there has also been a small jump. Now, the data of, say, Leverrier's Tables having been determined from observations dated in Besselian Time should be consistent with fresh observations so dated. When compared with those dated by the Observatory clocks they do not agree, there is an apparent error of the Tables, which is really due to the fact that we are comparing a theoretical state of things at a Besselian date with the actual state at a Leverrian date nominally the same.

The Epoch of Leverrier's Elements is Jan. 1·0 1800 in Besselian Time, which is not the same physical instant as Jan. 1·0 1800 of Leverrian Time or that of the present *Nautical Almanac*, and the Elements require correction for the difference.

* The Mean Year or Mean Interval between two passages of the Sun through the first point of *Aries* is determined by observation as 366 Sidereal days $+n$ seconds, and this necessarily equals 365 Mean Solar days $+n$ Solar seconds. If n be wrongly determined, the change will not be in the length of the Sidereal day and second, but in that of the Solar day and second.